

LETTERS TO THE EDITOR

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The Late Eclipse

AN answer to Mr. Proctor (vol. xiii. p. 186) is unnecessary to those who know all that has been written on the possibility of photographing the spectrum of the corona, but I take the liberty to give a few quotations out of the literature on the subject for the benefit of those who take an interest in the discussion, yet had no opportunity of following it in detail. Want of space prevents me from quoting all the letters in full, but I believe that I have not left out anything which might alter the sense of the quotations. The passages which seem to me to be important to the point at issue are printed in italics.

1. Letter to the Editor of the *Daily News*, signed Richard A. Proctor, January 26, 1875:—

"It is said that some enthusiastic students of science propose to try to get photographs, not of the corona as seen in a telescope, but of the exceedingly faint coronal image seen with a spectroscope. If they succeed they will have achieved a clever photographic feat, but the result, so far as the corona is concerned, can have little scientific value. *It is mathematically demonstrable that this is the case, for the quantity of light actually forming the coronal image can be shown to be far less in amount than is necessary for the formation of a satisfactory photograph.*"

2. Letter signed "A Fellow of the Royal Astronomical Society" (*English Mechanic*, May 21, p. 248):—

.... "But if Mr. Proctor should take upon him to answer the first of these questions in the affirmative, the second in the negative, then I could ask him whether any body or any set of men possessing the slightest knowledge of the subject could or would have issued the preposterous instructions about photographing the spectra of bright lines in the corona, which emanated from the Royal Society? The bright lines in the corona! Why, it has been recorded again and again by skilful observers that so faint is the light of the whole corona during the totality of a solar eclipse, that it casts no shadow whatever; and we know that the outer parts of the corona failed utterly to impress a collodion plate in five seconds, upon which a sharp and effective image of the partially eclipsed moon impressed itself in 0'1 second with a longer focussed telescope!"

(I should not have quoted in your columns any remarks of this anonymous writer had not Mr. Proctor's reference to them in the following letter rendered it necessary.)

3. Letter from Mr. Richard A. Proctor (*English Mechanic*, May 28, 1875, p. 272):—

"With respect to the eclipse observations last April, I have already said, as 'F.R.A.S.' does (let. 9, 113, p. 248), that the failure of the Government expedition was rendered certain by the instructions of the Royal Society Committee. I pointed this out also before the expedition started. *I agree with F.R.A.S. entirely in his interpretation of the matter.*"

Taking these letters in connection with what Mr. Proctor now writes, the following seems to be Mr. Proctor's opinion:—

It is mathematically demonstrable that "the quantity of light forming the coronal image is far less in amount than is necessary for the formation of a satisfactory photograph" (*Daily News*), yet "Dr. Schuster proves very readily that the spectrum of the corona can be photographed in one minute" (*NATURE*).

Mr. Proctor "is not aware that anyone has questioned the fact," but he "fully agrees with an anonymous writer that no set of men having the slightest knowledge of the subject could have issued the instructions which emanated from the Royal Society."

The failure of the Eclipse Expedition was rendered certain by the instructions of the Royal Society Committee to photograph in four minutes what Mr. Proctor admits to be capable of being photographed in one minute.

Anything which Mr. Proctor could have written on the subject besides what has been quoted, as, for instance, the passage in "Science Byways," he alludes to in his letter, can only add to the hopeless confusion which must bewilder anyone trying to form a correct and fair estimate of his view on the matter.

Everybody will agree with Mr. Proctor that such a controversy is not likely to be of any service to science.

The Fossil Skeletons of Le Puy en Velay

As there is to be a meeting of the Scientific Congress of France in Auvergne and Velay next summer, it may be useful to direct attention, through the columns of *NATURE*, to certain difficulties connected with the supposed antiquity of the fossil human bones preserved in the Museum of Le Puy.

With respect to the position of the bones, I visited the locality they were supposed to be found in last September, in company with some friends, and we were conducted, by the peasant who professes to have found them, to a well near the little auberge, where he now resides, and which is certainly a very different spot to that indicated by Mr. Poulett Scrope in his sketch at page 182 of the "Volcanos of Central France." The locality given by Mr. Scrope is much higher up the hill than is the well we were shown near the "Hermitage." Sir Charles Lyell also, according to the "Antiquity of Man," p. 229, was conducted to a spot "not far from the summit of the volcano." The well of the Hermitage is a long way from the summit of the hill.

It has long been observed that the rocky matrix in which the human bones have been enveloped is altogether different from the matrix of the rock where they are said to be found. This is certainly the case as regards the matrix of the rock in which the well is situated, which is a coarse volcanic breccia, while the bones lie in a volcanic sandy mass with a mixture of tuff and lime. I especially wish to direct attention to the position of one of the larger bones marked (I think) as an "iliac bone" in the Museum. The laminated mass between which it rests appears to me *stalagmitic*, as if these human remains had been washed into a fissure through which the water percolates downwards to the well of the Hermitage, and of which traces may be found higher up the hill.

I would also direct attention to certain stratified breccias near the western summit of the hill of Denise, which we thought looked more like the result of melting snow and the action of running water than of "volcanic alluviums," to which they have been generally attributed. These may be seen beyond the Croix de Paille on the road to Brioude high up on the flanks of the hill west of the volcanic outburst known as "The Chimney." The black and red scoriae shot out through this "chimney" cover the summit of the hill and overlie the stratified breccias. But these breccias are, if I read the geology of the district aright, the equivalents of those which, on the slopes of Denise, west of Polignac, have furnished the bones of the mammoth and tichorhine rhinoceros, and belong to glacial times.

The antiquity of the human skeletons must, I suggest, depend upon the correct determination of the spot where the bones were found. It is possible that they may belong to the age of the stratified breccias, and were washed into a crack or fissure during the Mammoth epoch, but they certainly do not look like it, if we may judge from the matrix in which they are enclosed. It is possible that they were enveloped in volcanic materials which were evolved during the last volcanic outbursts, for I believe that at Le Puy en Velay and in the Ardèches there have been eruptions of scoriae and ashes through volcanic vents and chimneys since the glacial epoch, when deep snows covered the summit of Denise in the winter time and the mammoth pastured in the vales.

W. S. SYMONDS

A Meteor in the Daytime

THE meteor referred to by the Rev. T. W. Webb was also seen at Dorking and at Southampton. The times given were "about 1.38 P.M." and "1h. 38m. 45s. P.M." Dec. 22. Mr. H. J. Powell, writing to me from the former town, says: "Its course was from S.S.E. to N.N.W., and it shot down the sky so—

It had no well-defined outline like the moon, but was merely an irregular luminous ball. Its size as compared with the moon was about one-sixth. Its motion was not a very rapid one, but more like a cricket ball (after it has been thrown) falling. I did not hear any sound after its disappearance." Mr. Powell, writing to the *Times*, also mentioned that it "left a long trail of fire behind it," and that the nucleus "broke up and disappeared before it had reached the horizon."

In the *Times* of the same date (Dec. 23), "F. W." writes: "In the full blaze of the sun—a rare sight in itself nowadays—I observed a bright meteor traversing the sky from south-west to north-east, in form like a common rocket."

These accounts no doubt refer to the same meteor as that observed at Hardwick.

WILLIAM F. DENNING

Tyndale House, Ashley Down, Bristol, Jan. 8

Blowpipe Analysis

MAJOR ROSS (NATURE, vol. xiii. p. 186) does not appear to have thought of the impurities his soda might contain in his test for the presence of a sulphide. Had he done so he would probably have remembered that all soda (unless specially prepared from sodium) contains traces of iron. This iron, on fusing with the sulphide, forms ferrous sulphide, which, as is well known, is soluble in fused sodium sulphide; and on adding water to the fused mass a black residue of ferrous sulphide remains behind.

Again, he says "there can be no room to precipitate anything in a drop of water," but surely this is erroneous. It is only a question of degree. Under the same circumstances a precipitate would be as certainly formed in a drop of water as in a gallon.

If Major Ross were to make allowances for the ordinary impurities of commercial reagents, a little more confidence might be placed in his tests.

T. S. HUMPIDGE

Marine Aquaria

WHILE reading Mr. Wills's very suggestive article on Marine Aquaria in your last issue, the following question suggested itself to me:—Does not the "larger proportion of carbonic acid in the lowest depths of the ocean" explain, at any rate partially, the formation of the "abyssal red clay," which Prof. Wyville Thomson has proved to occupy so large a portion of the bed of the Atlantic?

"Many of the insoluble carbonates, and in particular those of lime, magnesia, &c., may be dissolved to some extent by water, charged with carbonic acid, and are deposited in a crystalline form, as the gas slowly escapes from the fluid." (Miller's "Chemistry.")

That the presence of carbonic acid in the deep water is one cause of the decomposition of the shells of Mollusca, &c., I think that there can hardly be a doubt. Whether it is sufficient by itself to account for the whole phenomenon, I cannot presume to decide.

H. J. M. G.

Bournemouth, Jan. 10

The Glow-worm in Scotland

MR. J. SHAW's interesting note on the Glow-worm leads me to remark that it is common about Loch Lomond, and recalls the pleasant surprise with which I met one there, shining brilliantly by the wayside, so late as twelve o'clock on a dark midsummer's night.

WM. MC LAURIN

London, Jan. 10

Bryant and May's Safety Matches

THESE matches are highly electrical, and if they be rubbed against glass and ebonite they ignite, especially if the electrics be dry and warm. How far their ready ignition on amorphous phosphorus is due to chemism or to electricity remains to be proved. I am sorry I have not the opportunity just now to test this point.

W. H. PREECE

OUR ASTRONOMICAL COLUMN

THE MINOR PLANET, No. 153.—This planet, discovered by Palisa at Pola on Nov. 2, 1875, and which has been named *Hilda* by Prof. Oppolzer, is found to have a period of revolution considerably longer than any other member of the group. In No. 39 of Prof. Tietjen's "Berlin Circular" is an orbit calculated by Dr. Schmidt, which represents closely the observations to the end of the year. It is as follows:—

| | |
|--|----------------------|
| Epoch 1875, Nov. 22, at Berlin midnight. | |
| Mean anomaly | 108° 30' 11" |
| Longitude of perihelion | 284° 41' 50" { 1875° |
| Longitude of ascending node | 228° 20' 43" |
| Inclination to ecliptic | 7° 44' 52" |
| Angle of eccentricity | 8° 33' 3" |
| Mean diurnal motion | 452" 421" |

The major semi-axis is 3'9474, and if we calculate the

distance of the comet from the orbit of Jupiter at the aphelion passage, we find it 0'864, the earth's mean distance from the sun being taken for unity, which is a much closer approach to Jupiter's path than occurs with any other of the minors. *Themis*, for instance, the motion of which was investigated by Dr. Krueger, for determination of the mass of Jupiter, does not approach that planet within about 1'5. More than one of the small planets with the longer periods have large heliocentric latitude at the aphelion point, and do not on that account approach so near to the orbit of Jupiter as others with shorter periods and somewhat greater eccentricities, and having the lines of nodes and apsides less divergent. *Cybele* in aphelion is 1'31 from the orbit of the great planet, *Freia* 1'24, and *Camilla*, according to the rather uncertain orbits yet available, 1'36. Hence, as suggested by Palisa, his planet *Hilda* is well situated for further investigation on the mass of Jupiter by the perturbations of the minor planets; it is well known, however, that this important element in the solar system is now reduced within narrow limits of uncertainty.

The above orbit of *Hilda* is confirmed by another computed by Herr Kühnert of Vienna from a similar extent of observations. The period of revolution is about 2,865 days, or approaching eight years, contrasting strikingly with the period of *Flora*, which is only 1,193 days, or a little over 3½ years.

SATELLITES OF URANUS.—The following positions of the brighter satellites of Uranus are derived as before from Newcomb's Tables in the Appendix to the Washington Observations for 1873; they are for 11h. 30m. P.M. Greenwich time:—

| | TITANIA. | | OBERON. | |
|---------|----------|-------|---------|-------|
| | Angle. | Dist. | Angle. | Dist. |
| Jan. 15 | 23° 2' | ... | 31° 1' | ... |
| " 16 | 3° 4' | ... | 34° 6' | ... |
| " 17 | 347° 7' | ... | 43° 1' | ... |
| " 18 | 268° 7' | ... | 15° 8' | ... |
| " 19 | 212° 5' | ... | 27° 2' | ... |
| " 20 | 190° 1' | ... | 34° 6' | ... |
| " 21 | 169° 1' | ... | 28° 9' | ... |
| " 22 | 120° 1' | ... | 16° 5' | ... |
| " 23 | 45° 4' | ... | 22° 4' | ... |
| " 24 | 16° 9' | ... | 33° 4' | ... |
| " 25 | 357° 5' | ... | 32° 1' | ... |
| " 26 | 324° 2' | ... | 20° 2' | ... |
| " 27 | 245° 2' | ... | 18° 0' | ... |
| " 28 | 202° 6' | ... | 30° 5' | ... |

THE GREAT COMETS OF 1874 AND 1680.—Now that the orbit of the fine comet of 1874 (Coggia, April 17), determined from the observations in the northern hemisphere to the middle of July, has been shown by the southern observations extending to October, to require but small corrections, we may examine with confidence the path of the comet about the passage of the descending node, when it approached near to the orbit of Venus.

Employing the elements calculated by Dr. Geelmuyden, of the Observatory, Christiania, we have the following results:—

| Heliocentric Ecliptic Longitude. | South Latitude. | Distance of Comet from Orbit of Venus. |
|----------------------------------|-----------------|--|
| 299° 45' | 2° 17' 48" | 0'003655 |
| 299° 48' | 2° 24' 38" | 0'003181 |
| 299° 51' | 2° 31' 29" | 0'003323 |
| 300° 0' | 2° 51' 58" | 0'006372 |

Therefore, assuming the solar parallax 8"·875, with Clarke's semi-diameter of the earth's equator, the least distance of the comet from the orbit of Venus is found to have been 293,000 miles, or only about one-fourth greater than the distance of the moon from the earth.

A very celebrated comet, that of 1680, approached the earth's orbit within even less than this distance. From the definitive elements of Encke it would appear that in 92° 3' 5" heliocentric longitude, just before traversing the plane of the ecliptic, towards the south, the comet's dis-